THURSDAY, AUGUST 2, 1894.

LORD KELVIN ON GENERAL PHYSICS.[1]

Popular Lectures and Addresses by Sir William Thomson (Baron Kelvin), P.R.S., LL.D., D.C.L., &c. In three volumes. Vol. II. "Geology and General Physics." With illustrations. NATURE Series, pp. x. + 599, with index. (London and New York: Macmillan and Co., 1894.)

II.

AT the present time, when the need for a fully-equipped and well-manned National Physical Laboratory for expensive and for secular observations is sometimes discussed, it is interesting to quote from Lord Kelvin's Presidential address to the British Association at Edinburgh in 1871 as follows:—

> "The success of the Kew Magnetic and Meteorological Observatory affords an example of the great gain to be earned for science by the foundation of physical observatories and laboratories for experimental research, to be conducted by qualified persons, whose duties should be, not teaching, but experimenting. Whether we look to the honour of England, as a nation which ought always to be the foremost in promoting physical science, or to those vast economical advantages which must accrue from such establishments, we cannot but feel that experimental research ought to be made with us an object of national concern, and not left, as hitherto, exclusively to the private enterprise of self-sacrificing amateurs, and the necessarily inconsecutive action of our present Governmental Departments and of casual Committees."
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> "On the Continent there exist certain institutions, fitted with instruments, apparatus, chemicals, and other appliances, which are meant to be, and which are made, available to men of science, to enable them, at a moderate cost, to pursue original researches."
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> "The physical laboratories which have grown up [in Universities] show the want felt of Colleges of Research; but they go but infinitesimally towards supplying it, being absolutely destitute of means, material or personal, for advancing science except at the expense of volunteers, or for securing that volunteers shall be found to continue even such little work as at present is carried on."

And in connection with the still urgently pressing need for a systematic abstract of papers and of a central comprehensive report of annual progress in physics, such as is already satisfactorily accomplished by our friends the chemists, the following quotation from the same address is likewise of interest:—

> "A detailed account of work done and knowledge gained in science Britain ought to have every year. The *Journal of the Chemical Society* and the *Zoological Record* do excellent service by giving abstracts of all papers published in their departments. The admirable example afforded by the German *Fortschritte* and *Jahresbericht* is before us; but hitherto, so far as I know, no attempt has been made to follow it in Britain. It is true that several of the annual volumes of the *Jahresbericht* were translated, but a translation . . . cannot supply the want. An independent British publication is for many obvious reasons desirable. The two publications, in German and English, would, both by their differences and their agreements, illustrate the progress of science more correctly and usefully than any single work could do."

From the same address I cull the following detached morsels:—

> "Our knowledge of the dark lines is due to Fraunhofer. Wollaston saw them but did not discover them."
> "The old nebular hypothesis supposes the solar system . . . . to have originated in the condensation of fiery nebulous matter. This hypothesis was invented before the discovery of thermodynamics, or the nebulæ would not have been supposed to be fiery."

In amongst the more geological portion of the book there comes a Presidential address to the Society of Telegraphic Engineers, from which it may be useful to extract the following compact statements concerning atmospheric electricity:—

> "In fair weather the surface of the earth . . . . is always found negatively electrified. . . . The more common form of statement is that the air is positively electrified, but this form of statement is apt to be delusive. . . . . The surface of the earth is negatively electrified, and positive electrification of the air is merely inferential, ... the lower regions of the air [such air as comes in through windows] are negatively electrified. . . . . It is not always negative, however. I have found it positive on some days. In broken weather . . . . it is sometimes positive and sometimes negative. Now hitherto there is no proof of positive electricity in the air at all in fine weather; but we have grounds for inferring that probably there is positive electricity in the upper regions of the air."

Opening the book now at page 360, we find a paper which might well have been included in the volume on "Navigational Affairs," being on the subject of the Rate of clocks and chronometers as influenced by the mode of suspension. It is rather surprising to learn that the rough and ready conditions of a pocket by day and a pillow by night give a watch a better chance of going correctly than many other modes of support, such as hanging on a nail or even lying flat on a table. If a correctly-going watch be hung up by a single long thread normal to its plane it begins to gain, and if its case has n times the moment of inertia of the balance-wheel, it gains one in $2n$ swings; a watch actually tried, whose n was 650, gained more than a minute (67 seconds) in a day when so suspended. Suspend it by a bifilar suspension and gradually move the threads further apart, so as to increase the natural rate of swing of the case, and the watch gains more and more, until, when the periods of case and wheel coincide, it gains furiously, and then either stops altogether or else begins to lose equally furiously. Separate the threads a little more still, and the losing rate begins to diminish, until ultimately, when the constraint is great, it begins to keep correct time again. Thus by suspending a chronometer judiciously it can be adjusted to time without touching the hands; but if it be suspended so as to have a quick natural period of swing, it cannot be expected to keep good time. If placed on a cushion to protect it from jars, its case is not unlikely to have a quick swing-period;

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[1] (Continued from page 293.)

it should be firmly fixed to something with a considerable moment of inertia, and then placed on its cushion. Any fairly regular motion of the case is fatal to good going. And as to astronomical clocks, they should be fixed to stone piers with the same sort of care as is bestowed on transit instruments.

Another paper, "On a New Astronomical Clock," notes the defects of Graham's dead-beat escapement, and suggests a new one, wherein the escapement wheel is carried by a loose friction collar at a rate a trifle faster than the proper rate, so that its pellets engage the pendulum only occasionally, receiving the necessary check and maintaining the motion of the pendulum sufficiently, even though they only touch once a minute or so.

In a paper "On Beats of Imperfect Harmonies," of date 1878, Lord Kelvin virtually lends his support to the view advocated by Koenig, that in the appreciation of harmony the ear detects phase-differences and is not limited to analysis of a complex note into simple harmonic constituents. For instance, a harmony of even and odd vibration numbers (like 2 : 3) will have one kind of phase relation, while a harmony of two odd numbers will have another kind, the most obvious feature of these phase relations being the way successive maxima and minima coincide or oppose. In general the shape of the curve representing the composition of two notes varies in appearance, as is well known, according to the phase in which they are compounded. If one of the constituents is out of tune there will be a gradual transition from one of these phase-relations to another. "In favourable circumstances . . . a variation of the sound recurring periodically in the successive cycles is distinctly heard. . . . It is this variation which is called the 'beat' on the imperfect harmony."

Lord Kelvin has made experiments on pure tuning-fork tones, and his experience is "that in every case the ear does distinguish the two halves of the period of each beat. . . . The ear distinguishes the quality of the sound represented by the sharp-topped and flat-hollowed curve from that represented by the flat-topped and sharp-hollowed curve. In the one case the pressure of air close to the ear rises very suddenly to, and falls very suddenly from, its maximum, and (as in cases of tides in which there is a long hanging on low water) there is a comparatively slow variation of pressure for a few ten-thousandths of a second on each side of the instant of minimum pressure; in the opposite phase-relation there is a slow change before and after the time of maximum pressure, and a rapid change before and after the time of minimum pressure."

The ear is thus found able to distinguish between a push and pull on the tympanum; or the receiving apparatus is not symmetrical on either side of zero. This is equivalent to saying that second order of small quantities must affect the sound as heard, and on this can be based the usual theory of the difference and summation tones of Helmholtz.

But the mode of expression adopted by Lord Kelvin is not that of interference of any resultant simple tones; he prefers to think of the actual phase changes as directly detected by the ear, and says that "a *revolving character* which I perceive in the beat is to me certainly distinct enough to prove that the ear does distinguish between these configurations, which are one of them the same as the other taken in the reverse order of time."

According to his experiments it is singular how very faint is the disturbance necessary to bring out these beat tones: much less than would appear to be necessary on Helmholtz' theory of difference tones, whose amplitude is proportional to the product of the constituent amplitudes. Thus, "if when the approximate harmony C E is being sounded, with the E slightly out of tune and the beats on it heard, the faintest sound of G is produced by a very gentle excitation of the fork by the bow, instantly a loud beat at half speed is heard. . . . It is marvellous how small an intensity of the sound G is required to give a smooth unbroken loud beat in the double period." This practical method of tuning a major third, by addition of the minor third above it, completing the common chord, is of course well known; but the ordinary Helmholtz explanation, of beats between the C E difference tone and the E G difference tone, scarcely seems to fit the above observed facts.

Again, if the notes C E G are sounded and one of the notes (say C) is flattened, the beats are not only very audible but "the sound dies beating, the beats being distinctly heard all over a large room as long as the faintest breath of sound is perceptible. The smooth melodious periodic moaning of the beat is particularly beautiful when the beat is slow (at the rate, for instance, of one beat in two seconds or thereabouts), being, in fact, sometimes the very last sound heard when the intensities of the three notes chance at the end to be suitably proportioned."

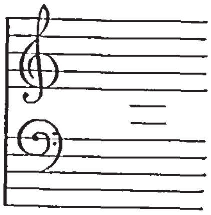
Incidentally an inconvenient usage of musical nomenclature is mentioned in a note. The word "tone," which is now coming to be used to mean a pure sine curve disturbance or simple note, means in music the interval of the major or minor or tempered second.

Those who have to do with acoustics must have often experienced the inconvenience of the ordinary childish nomenclature of intervals—a fourth, a third, a seventh, and so on—especially when these intervals are being numerically expressed at the same time. To call the interval 2 : 3 a fifth, 3/4 a fourth, and 1·25 a third, is often confusing. Might I suggest that these intervals, when true, might be named readily and intelligibly as respectively a do-sol, a sol-do, or if preferred a do-fa, and a do-mi; similarly a minor third would be a mi-sol; a major and a minor tone would be a do-re and a re-mi respectively; and so on.

It might be convenient to drop the *l* in sol, so as to make all the syllables of two letters; and then the flattening or sharpening of notes might be indicated readily by a final *a* or *e*; thus a flattened major tone would be a do-rea (the same as a re-mi), and a sharpened one a do-ree.

On the tempered scale the intervals could be called *cg*, *gc*, *ce*, *eg*, &c., with perfect ease.

Again, the ordinary musical notation, with its various clefs, if it were not hallowed by usage, would seem a barbarous piece of stupidity. Undoubtedly a couple of lines should have been understood as missing between the bass and treble clefs, instead of only one; so that the F label could be affixed to the top line of the bass instead of to the second from the top (see fig.); or rather, since the second line from the bottom is G, the same as in the treble, no labelling would be wanted, and one clef would serve for all instruments—a change which would surely save a Conductor something? I wonder if it is too late now to make the change! This, however, is a digression, and Lord Kelvin is not to be held responsible for any of these musical notation heresies.



The remaining parts of the book consist, for the most part, of Presidential addresses and a couple of Royal Institution lectures. The first R.I. lecture, "On the origin and transformation of motive power," is now of only historical interest. It is of date 1856, and in it the energy of motion is called "dynamical" or "actual" energy, though in a note the author says that he very soon after suggested the name kinetic.

It is followed by the address to Section A at York in 1881, on the practical utilisation of wind and water power; and then begin the quite recent articles. First, an article on the Dissipation of Energy for the *Fortnightly Review* of 1892, wherein the author points out how near Carnot was to an appreciation of the second law of thermodynamics, and justifies his own limitation of its statement to "inanimate material agency" by the following:—

> "My statement of this axiom was limited to inanimate matter because not enough was known either from the natural history of plants and animals or from experimental investigations in physiology to assert with confidence that in animal or vegetable life there may not be a conversion of heat into mechanical effect not subject to the conditions of Carnot's theory. It seemed to me then, and it still seems to me, most probable that the animal body does not act as a thermodynamic engine in converting heat produced by the combination of the food with the oxygen of the inhaled air, but that it acts in a manner more nearly analogous to that of an electric motor working in virtue of energy supplied to it by a voltaic battery. . . . It is, however, conceivable that animal life might have the attribute of using the heat of surrounding matter, at its natural temperature, as a source of energy for mechanical effect, and thus constituting a case of affirmative answer for Carnot's last thermodynamic question.[1] The influence of animal or vegetable life on matter is infinitely beyond the range of any scientific inquiry hitherto entered on. Its power of directing the motions of moving particles, in the demonstrated daily miracle of our human free-will, and in the growth of generation after generation of plants from a single seed, are infinitely different from any possible result of the fortuitous concourse of atoms."
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> "Considerations of ideal *reversibility* . . . have no place in the world of life."

In an address on the opening of the Physical and Chemical Laboratories of the North Wales College, Bangor, it is stated that there is no philosophical division whatever between chemistry and physics; both "investigate the properties of matter." I would suggest that properties common to many kinds of matter belong to Physics, while the properties whereby one kind of matter differs from other kinds belong to Chemistry. Of course there can be no sharp line of demarcation, but instinctively we are conscious of a difference; and wherever the investigation is concerned essentially with specific varieties of matter, it is felt that the interest attaching to it is a chemical interest. Properties of matter in general, in its different states indeed but without regard to whether the matter is pure or impure simple or compound—those usually belong to Physics.

In this Bangor address there are some interesting reminiscences of the old building of Glasgow University, and of the early days of students' laboratory work there, where under the inspiration of their unique teacher, even theological students worked away at practical experimental physics. And an excellent training too! The modern system of different curricula for each class of professional students, even in the early stages of their degree course, is probably not half so wise as the old Scotch system, where everyone had a year at natural philosophy as well as a year at metaphysics; and the course for everyone up to a degree standard was the same, whatever he was going to be. Specialisation at an early stage is now largely advocated, but I believe that our descendants will regard it as a mistake; or certainly that in effecting a partially required reform we are running now too far into an opposite extreme.

A good wholesome uniform range of subjects, with sufficient variety for different tastes but no reprieve from any course, is the best pregraduate course for all but intellectual weaklings; and for weaklings to attempt to specialise, as they sometimes do now, because it is easier to pass a high stage badly than a low stage well, cannot be really useful or satisfactory.

Those who are incompetent to go deep, and are necessarily superficial, let them try to give their surface breadth; and for those who can go deep, let them spread wide too. A liberal culture and wide information can hurt nobody of decent ability, and it need not be inconsistent with any depth to which a man's genius can carry him. True depth is an affair of genius. Training has chiefly to do with breadth. (This is another digression.)

Of the author's brief annual addresses as President of the Royal Society, the first is on the recently observed slight shift of the earth's polar axis, and on the Faraday centenary; the second on terrestrial magnetism, and the conceivable modes by which the sun may be able to disturb it. The following sentence may be quoted: "I find it unimaginable but that terrestrial magnetism is due to the greatness and rotation of the earth." And on the hypothesis that magnetic disturbances are caused by the direct action of the sun acting as a variable magnet, he says:—"In eight hours of a not very severe magnetic storm, as much work must have been done by the sun in sending magnetic waves out in all directions through space as he actually does in four months of his regular heat and light. This result, it seems to me, is absolutely conclusive against the supposition that terrestrial magnetic storms are due to magnetic action of the sun; or to any kind of dynamical action taking place within the sun, or in connection with hurricanes in his atmosphere, or anywhere near the sun outside.

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[1] This question was:—"Is it possible to derive mechanical effect from heat of average temperature?"

"It seems as if we may also be forced to conclude that the supposed connection between magnetic storms and sun-spots is unreal, and that the seeming agreement between the periods has been a mere coincidence."

The next year's address is on electric radiation, the experiments of Hertz, and electric discharge in gases; with a reference to Mr. Crookes' discovery of vacuum-stresses as an outcome of experimental troubles experienced in his weighing of thallium, and with a characteristic foot-note by Lord Kelvin to the word "troubles":—"Tribulation, not undisturbed progress, gives life and soul, and leads to success when success can be reached, in the struggle for natural knowledge." It is followed by the speech delivered at the unveiling of Joule's statue in Manchester Town Hall on December 7, 1893, and by the Royal Institution lecture on Isoperimetrical Problems; this last being an attempt at popularising the calculus of variations! From how to surround a maximum acreage with a given boundary subject to certain conditions, and how to plan a railway route with a minimum of expense, the author ascends to recent researches in the problem of three bodies, and to the geometrical representation of problems of dynamical stability by the method of geodetics. The most curious part of this lecture is not scientific but social, viz. the treatment accorded to that unfortunate hero, "Horatius Cocles." The representation of Dido as a cute Phenician adventuress successfully wheedling a reasonable plot of ground out of a sarcastic African chief is fair enough, but the spectacle of the stout old warrior with his wounded leg scrambling after a plough along a single furrow from morning till night over all kinds of country, in order to secure as much of the public cornland as possible at the hands of his grateful countrymen, is an odd reading of the legend. That he was awarded a piece of land such that it would take two oxen the whole of a day to plough it, is a statement poetic perhaps in its terms but more precise in its meaning than if expressed in some extinct units of measurement; but to suppose that it was to be ploughed round, and that Horatius must guide the plough, and guide it with a constant eye to secure the maximum of benefit for his minimum of service, is hardly fair either to the memory of the patriot or to the spirit of the Romans in their early and wholesome days.

It can hardly be said even now to represent the attitude of any nation with respect to the services of its military or political heroes, but it may very well be held as a typical illustration of the way in which most countries at present attempt to reward their inventors through the medium of their patent laws.

Whether the author half intended the Horatian episode as a satire, or whether (as is more probable) he is taking the story as a myth for whose social significance or historical bearing he cares nothing, it serves as a popular introduction to what else would be rather an abstruse subject—a subject, indeed, which few people would have ventured to use as the basis for a Friday evening discourse.

These, then, are the varied and highly readable contents of this small book. May the author long live with undiminished vigour, and give us many more of these recreations of a great mind.

OLIVER J. LODGE.

THE FLORA OF CEYLON.

A Handbook to the Flora of Ceylon: containing Descriptions of all the Species of the Flowering Plants indigenous to the Island, and Notes on their History, Distribution and Uses. By Henry Trimen, M.B. (Lond.), F.R.S., Director of the Royal Botanic Gardens, Ceylon. With an Atlas of Plates illustrating some of the more interesting Species. Part i. Ranunculaceæ—Anacardiaceæ. 8vo. pp. xvi. 327, with plates i.-xxv. (4to). Part ii. Connaraceæ—Rubiaceæ. pp. 392, with plates xxvi.-l. (Published under the authority of the Government of Ceylon. London: Dulau and Co., 1893-94.)

WHEN Dr. Trimen left England at the beginning of 1877 to undertake the directorate of the Ceylon Gardens, he had already formed the determination to elaborate the flora of Ceylon, and to publish a descriptive handbook of its botany. Those who knew him knew that this work would only be undertaken after due preparation and without undue haste, but that it would be pushed forward steadily and with all reasonable speed to a satisfactory consummation: and the two instalments now before us amply justify such a conclusion.

Dr. Trimen was fortunate in having had so careful a predecessor as G. H. K. Thwaites, whose "Enumeratio Plantarum Zeylaniæ," published in 1858-64, he rightly describes as "an extremely accurate and most valuable work," rendered more useful by the extensive series of illustrative specimens distributed by Thwaites to the principal herbaria of the world. The first work of the new Director was to bring this up to date, which he did in a "Systematic Catalogue," published in 1885, and arranged in accordance with the "Genera Plantarum." In the course of a visit to England in 1886, Dr. Trimen found time to examine the invaluable Ceylon Herbarium of Hermann, preserved in the British Museum, upon which Linnæus based his Flora Zeylaniæ; and he published a complete enumeration and identification of the plants therein contained, with notes, in vol. xxiv. of the *Journal of the Linnean Society.* Various new species have from time to time been published by Dr. Trimen in the *Journal of Botany;* and these, with the results of the rest of his work, are embodied in the "Handbook."

In his younger days, Dr. Trimen was known as a painstaking British botanist, and the "Flora of Middlesex," issued in 1869, for which he was mainly responsible, initiated a new departure in works of the kind. It was marked by thoroughness and accuracy; every page showed care and research: and these qualities are abundantly manifest in this Ceylon "Handbook." A careful correlation of the work of predecessors in the same field is another characteristic shared by each book; and in each there was need for this, for Middlesex plants have been recorded since the days of William Turner, while the Cingalese flora has been treated of by various authors from Hermann (1717) downwards.